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## I N Q U I R Y

I N T O

T H E N A T U R E A N D C A U S E

O F

F E V E R,

M O R E E S P E C I A L L Y

*T H E I N T E R M I T T I N G :*

C O N T A I N I N G

AN INVESTIGATION INTO THE NATURE OF MIASMATA,  
AND THE MANNER OF ITS ACTION UPON  
THE HUMAN BODY.

By  
W. Adams x

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Estimatio causæ, sæpe solvit morbum.

P R Æ F. C E L S I.

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E D I N B U R G H :

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1797.

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MED. CLINE



# DEDICATION.

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TO MY FELLOW MEMBERS

OF THE

ROYAL MEDICAL SOCIETY.

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GENTLEMEN,

*Edinburgh, April 1797.*

**N**OT now being in the habit of writing Dissertations for the Society, I could not of course have the satisfaction of submitting my subject to your examination, before I published it to the world; which circumstance, (though I lost by it one great source of information, and the power of correcting my opinion by the thoughts of others), yet I trust it has been attended with one good effect, as it has made me more anxious in my researches after truth from other sources, and perhaps not less cautious in adopting or forming opinions, before I was first led to do it from matter of fact. Not that I think what is contained in  
the

the following Work is incontrovertible ; for we know the soundest doctrine (in medicine at least) will always be open to some objections ; human knowledge, I fear, being too limited for us ever to arrive at perfection. Yet, such as it is, Gentlemen, from a confidence in your candour and ability, I resign very cheerfully to your investigation ; and am,

Most obediently,

And truly yours,

THE AUTHOR.

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## P R E F A C E.

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**I** AM fully sensible, how insignificant it would be to trouble my readers, at first setting out, with a long and tedious preface: Being conscious, notwithstanding all my endeavours to the contrary, some parts will be sufficiently tedious already, so that I have no doubt I shall have claim enough upon their indulgence, although I should forbear making any preface at all. I shall, however, just observe, (in excuse for any inaccuracies I probably may have run into), that my subject is new, and has led me into several little doctrines in physiology, which indeed I had no intention to discuss when I began, but to keep as close to the subject-matter of this Work as I possibly could: Yet having been as it were forced upon those things, I have examined and explained them to the best of my power; and when I saw they would take up too much room, I have (as I thought it would be more proper) reserved them for another Work.

With regard to the newness and importance of the subject, I think I cannot do better to illustrate that, than to quote the words of eminent men, who have done much in other parts of our science;

A

but

but (from some imperfect knowledge, I suppose, of the animal œconomy, or rather, I believe, from the infancy of other sciences from which we now derive great assistance), who have always been exceedingly puzzled, when they attempted to account for the cause of fevers, and especially the intermitting fever. Hippocrates supposed it might be owing to air taken into the blood\*. But Galen, though he sometimes speaks about the bile, and black bile, yet upon the whole, chooses rather to confess his ignorance, and say, he does not know any thing about it: Probably we might do the same, but I think some inquiry ought to be made; and if we have not been fortunate enough to succeed, I am persuaded, the time is not far distant when the mystery is to be unfolded;—the materials are all ready;—only there is one circumstance, which will strike the mind of a considerate person, after perusing the following Work. How can it be possible, that a matter so plain could have lain hid for so many ages? Surely the explanation of it ought to have been a matter of greater difficulty? This circumstance, I own, makes me quite dissatisfied with my labour. The cause of the disease appears very simple; and the variations of it also seem to me to be caused by very simple means. So that, it will appear, admitting

\* See his Book, *De Flat.*

admitting what I have here to offer actually is true, the discovery may be important, and yet at the same time the discoverer, I suspect, cannot be entitled to much praise.

A very candid Physician, of more modern times, has proposed several questions concerning the causes of this disease, and of its symptoms, out of which we may take our choice. “Are the solids morbid? Is the blood vitiated? Is there any morbid state of the stomach, liver, or bile? Is the sweat discharged at the end of every paroxysm, possessed of any morbid quality? Does the proximate cause reside in the nervous influence? How are the symptoms connected? What is the cause of the regular return of the paroxysms?” (See Gregory’s *Elements of Physic*, § 52.)

And another very able Author, of still more modern date, after delivering it as his opinion, that the disease proceeds “from a deficient secretion of the sensorial power, from the inaction of the brain;” and that the cause of that, “is a death or torpor of some part, as of the kidneys or the spleen;” asks, whether the revivescence of these affected parts, recurring at intervals, forms the paroxysms? and their permanent revivescence the cure?” (Darwin’s *Zoonomia*, Febr. inirritativa.)



Boerhaave seems to have been accurately acquainted with all the symptoms, and sometimes to have guessed very shrewdly at the cause of them, (see § 755.); though I believe he does not appear to understand very well the cause of the disease itself. He says, “ After a due examination of the whole  
 “ history of intermitting fevers, the proximate  
 “ cause appears to be a sluggishness of the arterial  
 “ fluid, and perhaps an inactivity of the nervous  
 “ juice of the brain, and cerebellum destined to the  
 “ heart ;—then comes some cause which excites a  
 “ swifter and stronger contraction of the heart, and  
 “ a resolution of the matter *which stagnated.*”

Van Swieten, his great, and I believe I may add, very valuable commentator, does not much trouble his head with forming conjectures about the matter : He sometimes talks about “ the predisposing  
 “ cause, whatever it may be,” &c. and is inclined to think, it is to be found in the nervous system. (Com. upon App. 757.). But with respect to the reason why the fits return at certain periods, he plainly says, that “ it is a matter of great difficulty ;  
 “ Physicians in general having tortured themselves  
 “ about it to no purpose.”

Dr Cullen also, though he has himself wrote about it, confesses with much candour, in many places, that the knowledge of fever in his time was very incomplete ; which he attributes rather to

our imperfect knowledge of the laws of the animal œconomy, (it being certain they ought to be well known, before we can explain with clearness the cause of any disease). And he points out a mode of inquiry in the Preface to his Work, where he particularly recommends the study of these laws to any one who undertakes to investigate the diseases of the human body ; adding, “ that it may be difficult ; but it must be attempted, or we must desert the subject altogether ;” and I think he seems to be pretty right.

We shall now just give a view of the method we have followed. In the first place, we have endeavoured to ascertain what was formerly meant by “ Miasmata,” what that matter is which goes by this name. Then we have shewn, (availing ourselves of the observation and experience of others), the peculiar method in which it operates upon the animal machine. Lastly, The state of the body immediately induced by “ Miasmata” is examined, and the manner in which this brings on those regularly returning fits of an intermitting fever. There is a short Dissertation also, upon the proper mode of cure ; but the nature of that, together with a few other things which I need not mention here, will best be seen by referring to the Work.





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## CHAP. I.—SECT. I.

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### *Of FEVERS, more especially of INTERMITTENTS.*

**T**HERE have many theories been formed concerning the cause of fever ; yet, it is allowed, all seem liable to considerable objections.

The latest of these is Dr Cullen's, which, it must be confessed, shews much ingenuity ; but in which, like the rest, on strict examination, one cannot find that consistency and uniformity, which, I think, are discernible in all the operations of nature. Indeed the arguments in support of it are not sufficiently satisfactory, of which the respectable author himself seemed perfectly aware.

The following is an attempt to account for the cause and symptoms attending fever, upon principles somewhat different. And there is no doubt, when the cause of fever is well understood, and the phenomena observable in the course of it rationally accounted for, we may then easily devise the most proper method of cure.

I would not, however, have the reader here expect a masterly performance in this little work; I mean it only as a sketch or general outline, to be approved or condemned, as the judgement of mankind shall think fit: If true, it is only a collection of general ideas, to be improved, perhaps, or arranged some other time, or by some other person.

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## S E C T. II.

WITH respect to the cause of fever, authors, as was said before, have entertained different opinions: Some supposing it arose from a kind of poison, received into, or formed in the body; and others, that an alteration in the consistences, or force of the blood, was the sole cause. All seemed nearly to agree in this, That the most violent symptoms were not immediately the effects of the disease, but of the *vis medicatrix naturæ*, endeavouring to overcome it. Even Dr Cullen himself yielded to this opinion; and indeed his reasoning upon the subject cannot be said to be by any means uninstruative; or void of entertainment.

As to the two first of these suppositions, (for so they are entirely, unsupported by matter of fact), they have been, I think, sufficiently well discoursed upon elsewhere. I have only now to remark, that what are said to be the causes of the disease, are not in many cases to be observed, till after the disease has taken place. No cause has, or perhaps can be, assigned

assigned in the greatest number of cases for this alteration in the blood previous to the disease ; I mean with respect to its viscosity. And it still more frequently happens, that no method can be thought of, by which any poison could be collected. But even allowing these things to exist before the disease, still they must have a cause. It is only passing over the subject, not examining it, to make mention of them. However, setting aside all this, I cannot think they are adequate to the task assigned them, viz. of producing those regular returns of an intermittent, or even the less varied phenomena of continued fever ; since we see each of them do occur, and are succeeded by diseases very different.

I next must beg leave to consider Dr Cullen's, which also is hypothetical, and, with regard to plain demonstrative evidence, stands upon no better ground than the former.

The first perceptible cause of fever, he believes to be " injury of the nerves caused by contagious matter." And this again produces a debility of the whole system, *especially of the extreme vessels of the skin.*" (See § 46. and 86). Now, it is very true, that always before the commencement of fever, there is a debility, that is, lassitude and uneasiness of body. But as (from the state of chemical knowledge at that time) this respected author could not say in what manner that lassitude or debility was produced, he was under the necessity of imagining it was done by contagion, or by miasmata, cold, or fear.

B.

According

According to my judgement, miasmata and contagion (according to the light in which they are generally understood) must be substances of an acrid, penetrating nature, something resembling the matter of small-pox, otherwise they could not be capable of inducing fever, by their quality of bringing on a putrefactive fermentation, or what not, in the blood. But, if so, their sedative power, I should expect, must be relinquished. For I hold it inconsistent with what we know of medicine at present, to say what causes such a violent commotion in the blood should immediately prove sedative.

An irritation would be the first consequence, and then, undoubtedly, both before and after putrefaction was begun, the sedative effects would follow. But it was necessary the first effect should be sedative, because experience shews it is so; and also because it produces a "debility of the whole body, particularly in the action of the extreme vessels." And then, "such is the power of the animal œconomy, that this debility proves an indirect stimulus to the sanguiferous system," (§ 46.) I shall not here say much concerning this, as I believe it is always easier to overturn an old theory, than to establish a new one; I shall, however, just remark, how inexplicable it is to me, that debility, or want of influence in the nerves, should so operate as to cause so violent an increase of action in the arterial system. And again, it is no less wonderful how that debility, or want of influence in the nerves, should immediately proceed from the irritating substance of contagion, or  
marsh



marsh miasmata. But we shall hereafter have occasion to give an opinion of our own ; and therefore, it is presumed, it would be better to be concise, and leave it to the judgement of others to determine which is best.

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### S E C T. III.

THE next step of the disease, or rather of the vis medicatrix naturæ, opposing it, (according to Dr Cullen), “ after the action of the heart and larger arteries is increased, is, to restore energy to the brain, and to extend this energy to the extreme vessels of the skin ; thereby restoring their action, and overcoming the spasm ; upon the removing of which, the excretion of sweat, and other marks of the relaxation of the excretorus, take place.” So that, in a few words, the disease, and all its symptoms, are attributed by Dr Cullen to something which weakens the energy of the brain, and to the attempt which nature makes to restore this energy.

There is another theory, I find, just published, concerning fever ; but in fact being so nearly like Dr Cullen’s, it is in most respects liable to the same objections.

## CHAP. II.—SECT. I.

*Of the CAUSES of INTERMITTENT FEVERS.*

**M**ARSH MIASMATA is now universally believed to be the cause of this species of fever ; but before we receive the opinion, it is necessary to inquire what that is.

In fens, and low places where there is much water stagnating, it is always observable, there is sure to be a frequency of intermitting fevers ; and therefore it has been said, that the air of such places contains a large quantity of infectious matter ; such, I presume, they mean as that causing small-pox. But, as I said before, such a matter would cause an inflammation in the part it is first applied to ; which is here not the case. Therefore analogy in this respect can have but little weight. It is unfortunate for those who made the supposition, that fever in all cases is caused by infectious matter ; because we are certain it is in some, that they did not duly consider that there is a considerable difference between them.

Fevers arising from contagion (I mean where their cause is quite evident and indubitable) always being capable of propagating a disease *sui generis*. But fevers not arising from contagion, (at least where their cause is very doubtful, and probably where it would not be judged to be contagion, could

could any other cause be rationally assigned), these, it is well known, do not very often (perhaps I may say never, but that will be considered hereafter) communicate the disease in the same manner; yet I allude here more especially to the intermitting and common inflammatory fever.

Again, fevers positively caused by contagion, always terminate in a discharge of the contagious matter externally; or when that is not discharged, in death; but these fevers very seldom do, and are often cured by bark, without any eruption or preternatural evacuations. What is called the military eruption, with a few others of the like kind, are, I am persuaded, very often nothing more than affections of the skin, caused by its being much irritated by the fever and external covering; and therefore much easier injured, or corroded, as it were, by the vitiated and profuse sweats, always preceding such eruptions.

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## S E C T. II.

It being somewhat doubted, then, whether intermittents are caused by any matter that is putrid, or infections floating in the atmosphere, (yet we judge the atmosphere may contain something which does cause them), by Dr Cullen called miasmata; we shall proceed to examine what that matter is.

It is still more improbable that that matter is contagious, which arises from the putrefaction of animal

mal and vegetable substances; because we see people exposed to it, for instance, in large fields of putrefying vegetables, and in dissecting rooms, in much larger quantities than can possibly arise in marshes, and yet no such bad effects ensue.

But to proceed: Chemistry has shewn us, the only pure air, fit for the support of animal life, is oxigene. Now, if it can be shewn, that that at any time is deficient in the atmosphere, or that the atmosphere is loaded with an unusual quantity of other vapours, I think it will appear some very important changes must ensue, (because the respiration of such air is continual); death itself may very often be the consequence.

But before entering upon the consequence of such a thing, it is necessary to prove such a thing exists, and to investigate its cause; which we now proceed to do.

In all places where there is a large quantity of stagnating water, or of putrefying animal or vegetable substances, as in marshes, there must be a copious discharge of elastic gases. 1st, From stagnating waters.

The component principles of water, it is well known, are oxigene and hydrogen; but it is seldom found perfectly pure, generally holding in solution a certain quantity of extraneous matter. When that matter happens to be calcareous, being held in solution by the acidifying principle of the water, I judge it not improbable, that that calcareous matter may still attract the oxigene, while the hydrogen, being



being more volatile, and acted upon by the heat of the sun, is disengaged. But the more common way in which water is decomposed, is by the action it has upon the animal or vegetable substance it contains. And what here may be said concerning the decomposition of water, containing only a small proportion of animal and vegetable substance, may also be applied to the decomposition of animal or vegetable substances, containing only a small proportion of water; or *vice versa*.

These compositions send out quantities of elastic fluids, different however in their quantities, and in their quality, according to the nature and proportions of the substances composing the mass.

It is evident, there is always a large portion of hydrogene emitted, from its being seen so often in those places, in the form of *ignes fatui*, or shining vapours. And it is evident, it must be compounded with other substances, (with phosphorus in general), or it would be more rarified and invisible.

But, independent of this, we have another proof of the decomposition of water; for the hydrogene may be collected. But I believe it is not now so much a doubt, whether these substances may be decomposed, as it is in what manner they are done; and water has been so little disputed on this head, that the decomposition of it has been assigned as the cause of the decomposition of the other bodies.

Animal and many vegetable bodies are composed of different proportions; of hydrogene, oxigene, carbone, and azote: But in many vegetables, this last principle

principle is wanting. Sulphur and phosphorus are sometimes found ; but I am doubtful whether we ought to rank them in the number of simple substances, or consider them as compounds ; either way, however, will do for us ; and to avoid running into prolixity or dispute, we shall prefer the former.

Oxigene is found to have a stronger attraction for carbone, than for any other substance : Charcoal will even decompose water, by the attraction of these two bodies. Hence arises that copious discharge of carbonic acid, from all putrefying substances ; and also, the enfranchisement of hydrogen.

It has been said, that when Nature makes a body, she always mingles in it a germ of destruction \*. But if that was the case, one should expect, as soon as life was departed, it would infallibly destroy it. Instead of which, it only acts under certain circumstances : for when varnished, or even kept in a dry place, or in spirits, they may be preserved a considerable time. The fact, I believe, is, they will not putrify at all, except they come into contact with oxigene.

This oxigene, either of the air or water, having such a strong attraction for carbone, unites with the carbone contained in dead bodies, and immediately forms carbonic acid. Thus the atmosphere (when it is the agent) appears vitiated in a double manner : First, by the abstraction of its oxigene ; and, 2dly, by the changing that oxigene into carbonic acid.

\* Fourcroy, (in his Philosophy of Chemistry), who mentions several circumstances well worthy of attention ; which I need not now repeat.

acid. If water is the agent, the atmosphere must equally suffer, from the disengagement of hydrogen.

By this absorption of the oxygen, the texture of the bodies is destroyed; and if they lie in water, they rise to the top. This circumstance has been attributed to the fixed air in the intestines being set at liberty; but as we have no proof of that, and it never happening till the moment they begin to putrefy, I think we may with more reason attribute it to the fixed air formed by the putrefaction.

As the air is lighter than water, and these bodies possessed of considerable gravity, we must not, look for them rising to the top of it; at least, I mean in their present form. But, as I said before, having in their composition several kinds of aerial gases, and these gases (hydrogen and azote, carbonic acid must here be excepted), being much lighter than the air, have a tendency to rise by the power of gravity; they are attracted sometimes also by the surrounding bodies; but that is not an accident which generally happens. They always have their attraction much diminished, for the now putrefying bodies; because the carbon is become saturated, as it may be called, with oxygen; and the principle of life is departed: both which, as relating to these things, may be considered in the same light with quiescent attractions. So that, in a few words, the power of gravity, assisted by the action of the sun, and the acquired heat of the putrefying body, rarifying the fluid it contains, we may look upon as the divellent attractions; and those we before men-



tioned, as the quiescent. It will easily be conceived, then, that the divellent attractions must prevail;—and the decomposition, in consequence of it, is what is called the putrefaction of animal or vegetable bodies.

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### C H A P. III.—S E C T. I.

**S**EEING, then, that there is such a quantity of elastic fluids, raised by putrefaction, it is presumable, that what was formerly attributed to marsh miasmata, may be caused by them; especially if we can shew they are capable of doing it; and that we shall endeavour to do very shortly.

The most noxious of these vapours, are carbonic acid, hydrogen, and azote; and these are all capable of permanent suspension in the air; though the sulphur and phosphorus, &c. are sometimes raised, and turned into sulphuric and phosphoric acid.

These, it is true, may be called marsh miasmata, if we please; but as they are found in other places, as well as in marshes, and when applied in the same manner, cause the same disease, *cæteris paribus*, I think it would be better to call them by some other name. Their common name, noxious vapours, or unwholesome gases, or any thing which conveys a clear idea of their nature, I take to be as good as any.

But

But it is as true that these gases do not always produce intermittents, when they are respired, as that intermittents sometimes are produced by other causes: the immediate cause of the disease is derangement of the circulation; and the tone of some people's arterial system is so firm, or, in other words, their constitution is so good, they are not so much affected by the impurity of the air.

But this very much weakens the theory of contagion: For if the cause of fevers was to act by virtue of this power, we should certainly find, that where it was most predominant, (as in dissecting-rooms, and other such places where it most undeniably is), it would take effect, as sure as ever it came into contact with our bodies.

Instead of which, we find that many can stand its utmost violence, provided they leave it at intervals; and most of us can bear it two or three hours in a day, when we accustom ourselves to it by degrees, and experience no feverish effects;—yet, such is the nature of this contagion, (according to the common established opinion of it), that when diluted, I may say, and rendered imperceptible to the senses, by being mixed with several times its weight of air, it was even then supposed capable of communicating fever by contagion.

Other things still remain to be said about this matter; but as we trust we have said enough for the present purpose, we shall beg leave to consider them another time.

And again, it is not requisite, that all these gases  
C 2
should

should be united : For Dr Franklin relates, he was attacked by the ague, from the respiration of hydrogen alone, which he was subject to for a continuance, from collecting at different times that fluid from stagnating waters. I also met with an accident of the same kind, from breathing a kind of hydrogenous gas, raised by putting iron-filings into diluted vitriolic acid. The fits were not very regular, nor severe ; but after I had been exposed to it, (which was two or three hours in a day), and some time after, I felt all the disagreeable symptoms of the cold stage of intermittents ; and after that, in a slight degree, succeeded the hot one. It did not however last long, as in two or three days the cause was removed : And the person assisting me was soon after attacked with the palsy ; but he was much accustomed to drinking.

It may be worthy of some notice, that whilst I was writing this, (being before in some expectation of the disorder), the cold fit was actually brought on by the same cause.

From this circumstance, (his accident), Dr Franklin concluded, all agues might be caused by this gas : And I find a celebrated American Professor is inclined to believe almost all kinds of pestilential diseases, said to arise from contagion, proceed merely from an union of azotic gas and oxigene. But as other gases are raised from the same source, and generally united with it, it would, I think, be unjust to deny them a share.

Agues also may proceed from the passions of the  
mind,



mind, at least one regular succession of the fits; (but for their continuance, there is also required some predisposition from the impurity of the air), or from any thing which suddenly checks the circulation of the blood. We have many instances of fear or surprise, or any depressing passion bringing on the cold fit of an intermittent. And we have it upon record, that certain people have been visited by it once a-year, or once in seven years. And it may be true; for probably at those times their mind was acted upon by some dejecting passion, so as to deny to the nerves their usual influence to carry on the circulation in a regular manner.

For it will be understood, that the nerves are primarily affected, and the circulation by them. But by far the most numerous examples we have of intermitting fevers, may be traced from this great cause, the respiration of impure air, generally vitiated by the admixture of certain quantities of the afore-mentioned elastic gases.

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## S E C T. II.

To shew this more clearly, we shall take a view of the first approach of an intermittent, as characterised by Dr Boerhaave, in his Aphorisms, (see § 749.). It is marked by “a yawning, stretching, weariness, weakness, coldness, trembling, shivering, and paleness of the extreme parts.” To  
which

which I may add, a sense of weight and oppression about the thorax, which practitioners allow to be a very common occurrence, and which I have myself many times observed. These evidently indicate an affection of the nerves. Then the same author immediately adds, "The respiration becomes very difficult." The cause of this, every one will readily admit, must be sought for in the lungs. Accordingly, he informs us, in a quotation from Harvy, the happy discoverer of the circulation of the blood, (see chap. xvi.) "Upon opening the bodies of those who had died in this cold fit, or first stage of an intermitting fever, after difficulty of breathing, sighings, and aversion to move the body, there is found *thick blood impacted in the lungs.*" This fact is taken notice of also by Van Sweiten, in his Commentaries, by Hoffman, Morgagni, and Hippocrates.

Hoffman, in his book, De Pleuride, mentions it as a circumstance very generally taking place, for the lungs to be affected by all acute disorders, and he quotes the authority of both Greek, Roman, and Arabian authors, who had had the opportunity of convincing themselves by dissection: He himself too, after relating the case of a person who was attacked "primum horrore et rigore, corporisque lassitudine, quæ symptomata mox subsequantur dolor in pectore, spirandi angustia, cum prostratione appetitus virium et caloris," &c. gives us the appearance upon dissection, "aperto caveris thorace, in conspectum pulmones veniebant, *mirum inflati distenti et tactu duri; et ferme totam thoracis cavitatem implebant:*



*implebant* :—*præteria erat memorabile* (he says)  
*arteriam et venam pulmonalem circa confinia cordis*  
*continuisse polyposas concretiones.* Presently after  
 he endeavours to explain what was thought so wonderful ; but instead of recollecting the stimulus the lungs receive from respiration, he has recourse to his favourite notion of spasm, or constriction of the skin. His words are, “ *Si sanguinis copia accrescit,—et*  
*præsertim, si (ex partibus exterioribus constrictis)*  
*regurgitet ad interiora ; ubi circa cor et pulmones*  
*congestus, cum non possit liberè transire,—precipue,*  
*si in venis polyposa hæreat substantia, stagnans et accumulans ; inflammatores tumores, &c.*  
*excitare poterit.*” It is impossible to quote all the authors who have furnished us with facts sufficient to convince us,—or who were indeed themselves convinced by facts which they have not communicated, that it is the preternatural accumulation of blood about the heart and lungs which gives rise to those distressing symptoms of the cold fit : many have known, that (but what may seem wonderful) they all had very imperfect notions of the cause of this accumulation, and its manner of producing those symptoms : when indeed they actually make mention of the cause, it seems only to be by chance, or as if they were forced to do it by its being so plain : they always, however, have some favourite theory to prevent them paying it proper attention : Yet Hoffman says \*, (after describing the effects bad air has in relaxing the body), “ *Si pulmonis vescicu-*  
*las,*

\* De Aeris intemperie, § 6. p. 450.

“*las, relaxat, ingens fit sanguinis ibi decubitus—unde*  
 “*respiratio fit anxia, et citatior cum dolore.*” He  
 goes on, “Totum porro corpus fit hebes, fit langui-  
 “dum, ad labores sustinendos minus aptum; appe-  
 “tentia decrescit,” &c. He says also, (but it is pro-  
 per to recollect, that, though such air may seem  
 cold, when we judge of it by its effect upon our  
 bodies, it very often in reality is not so), “Pessi-  
 “mum ille aër qui frigidus est et humidus: talis  
 “enim ubi perveniat ad interiora corporis, ad stag-  
 “nationes, stases sanguinis et humorum inferendas,  
 “est aptissimus.” He gives us facts that enable us  
 to draw conclusions; but I have never found him at  
 all inclined to make such conclusions himself, under-  
 taking rather to explain those facts by other means. In  
 another paragraph, likewise, Morgagni says, “Pulmo-  
 “nes durissimi gravissimi et distenti,” or some other  
 words to the like purpose, in almost every dissection  
 where the disease was attended with cold, shivering,  
 difficulty of breathing, or pain and oppression about the  
 thorax. Sometimes he says, “Ut totum thoracis ca-  
 “vum implerent\* ;” and “polyposæ concretiones,”  
 are very common words. The following is one  
 example, in the case of Faber Murarius: “Huic se  
 “addiderunt periodice quotidiani rigores.”—“Pul-  
 “mones gravissimi distenti, &c.; et polyposæ con-  
 “cretioneseductæ sint, non modo e cruralis venis,  
 “sed etiam e corde.” And here is another †, a man  
 having

\* See his Book, De Morbis Thoracis, Epist. xxi. Art. 6.

† Epist. xiv. Art. 4.

having difficulty in passing water, had also “ magna  
 “ quotidie febris, rigore et frigore, ad duas horas du-  
 “ rantibus ;” upon dissection, “ In thorace pulmones  
 “ turgebant, atroque erant calore infecti. Cor-  
 “ dis ventriculus dexter polyposam concretionem  
 “ habebat; sinister fluido sanguine erat plenus.”  
 Van Sweiten does not seem to have attended very  
 assiduously to dissections, so as to give us the histo-  
 ries of what he had seen ; but he frequently men-  
 tions it as a matter of fact ; therefore, no doubt, he  
 had it from good authority. In his Commentary up-  
 on Aphorism, 576, he says, (when explaining the  
 cause of the paleness of the skin), “ The force of the  
 “ heart is weakened from some cause, so that it is not  
 “ able to propel the blood to the extremities of the  
 “ body, the arteries at the same time contract at  
 “ their extremities, by their own elasticity ;” and  
 then “ the blood is driven from the smaller into the  
 “ larger branches:”—“ hence it will follow, that ex-  
 “ ternal cold may increase the paleness in the cold  
 “ fit of fever.” Still more plainly and pointedly,  
 a little after, he says, “ In the greatest severity of  
 “ the cold fit, the blood begins to stagnate in the  
 “ veins for want of the motion of the arterial blood,  
 “ *and from the larger veins and sinuses about the heart,*  
 “ *with the whole lungs, being overcharged with blood.*”

The following passage is to be found in the works  
 of a late and much respected Professor of Medicine in  
 this University, Dr Gregory : “ On dissection of  
 “ those who have died in the cold fit, there have  
 “ been found, *accumulations of grumous blood about*  
 “ *the heart and lungs ;* and when the disease has



“ *continued long, the abdominal viscera have been found enlarged, and diseased in different ways, the intestines distended with air, and overflowing with bile. When the disease has been of short continuance, the viscera have commonly been sound.*” (Elem. of Prac. § 51).

All these things, I judge, are sufficiently strong to make the fact we here allude to very certain ; yet we may meet with a great many more that tend as powerfully to prove the same thing ; only it would waste our time, and (as the thing is already proved) could not answer any good purpose ; but if any one chooses to know more about the matter, he may consult the Reports, Observations, and Instructions, of the different Humane Societies, established for the recovery of drowned or suffocated people : wherever any experiments or dissections were undertaken, that circumstance we have before mentioned was uniformly found.

It is indicated also, by the oppression and straightness so frequently complained of in the thorax, and difficulty of breathing, of which it is the cause. It is mentioned in the same quotation from Dr Harvy, “ the pulse was then always small, quick, and irregular\*.” This (notwithstanding all that is imagined to the contrary), must be caused by a larger quantity of blood being retained in the lungs ; for the flow of blood into the heart, and the consequent contraction of the heart, must ever be in proportion to the quantity and quickness with which it is driven from the lungs. [This

\* In this chapter it is evident, Harvy had penetrated some depth into the cause of the disease ; what he knew he had learned from dissection ; and, had he not been engaged by other matter, there is the greatest probability he would have fallen upon the same conclusions which we have here been induced to do,

This is founded on the lungs being the origin of the circulation, which we shall now endeavour to prove; and in so doing, we shall probably shew the cause why the blood is obstructed there, and consequently the cause of intermitting fevers.

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### S E C T. III.

EVER since the time of Aristotle, and probably a long time before, there have been many who had full conviction of the importance of the lungs, and believed there was something taken in there for the support of life. Aristotle calls it the vivifying spirit, and Hippocrates the food of life. Fornelius, an author of the sixteenth century, gives it pretty nearly the same name. But none ever went so far, as to pursue this subject by experiment, till the time of the late unfortunate M. Lavoisier; he even attempted to calculate the quantity, and shew us what it is.

Sir Isaac Newton, too, was of that opinion; and indeed, though he cannot perhaps be properly called the discoverer of oxygene, &c. because it does not appear he ever proved what he seems to have been so well acquainted with, since his time, we have got to greater certainty, though (in this particular) we have not advanced in height. He was of opinion, "That the action of the heart was continued by something of *an acid quality in the air, received into the blood by respiration.*" (See his Optics, 8vo edit. p. 355).

Some, however, who lived between the times of those renowned authors; as well as others of no inconsiderable eminence, who are alive at this present time, take very little notice of this operation in the lungs, but attribute most of the effects justly due to it to an imaginary power, they suppose lodged in the heart. We shall examine their arguments in support of such an opinion.

They tell us, 1st, That as much blood is sent into the lungs, as is circulated in the whole body besides; and as the lungs are only 1-15th part as big as the whole body, the force with which it is driven forward through the lungs must have been 15 times greater, if each side of the heart had been endued with equal force. But as the right is supposed only one third as powerful as the left, they are content to divide 15 by 3, and so make the force with which blood is circulated in the lungs, only 5 times greater than it has in circulating through the rest of the body. This calculation, founded upon a thing they were going to prove, is, they imagine, sufficient to account for the increased rapidity which blood is known to have when it gets into the lungs. Yet, as it is quite evident from the pulsation being nearly equal, and at the same time in all other parts of the body, and from several arteries being sent off at acute angles, that each artery has the power of continuing the impulse given by the heart; this calculation can have but little weight. If any thing is to be learned from it, it is this, that blood actually has a slower motion the nearer it approaches to the lungs

or heart; which is certainly the case, but not proceeding so much from want of assistance from the heart, or from want of power in the right ventricle, merely from a deficiency of that stimulus, which it is going to the lungs to receive.

Next they tell us, that amphibious animals have their circulation carried on, though they live a long time under water. This reference to these animals is very unfair, when we are going to decide the importance of respiration, with respect to man. Different animals have certain peculiarities; and the internal structure of these animals is very different from what we meet with in the human species. Besides, their temperature also is very different, which is caused by these peculiarities. They have an immediate communication between the two ventricles, and some of them have only one ventricle: So that the blood may either pass directly through the heart, or be circulated in the lungs, as occasion may require.

When they suspend their breath, it undoubtedly takes the first course: But then it is not capable of continuing in it long; and its motion also, I have no doubt, is much retarded. The animal is under a necessity of coming up frequently to respire; so that I think if either side may profit by these animals, they seem to shew the necessity of respiration.

Fishes live by the abstraction of a certain air from the water; and when it is deprived of this air, they die: as the famous Du Verney many years ago demonstrated. Yet their circulation is very different from ours; and their not being exposed so much to the

the



the atmospheric air, I suppose, is one reason why some of them have such long life.

There is another argument made use of against the necessity of the lungs for circulation, viz. that the circulation is carried on in the fœtus, though it has never breathed. But I would have it here recollected, that the mother has lungs, and that as her blood could not be circulated without their use, so I should suppose, upon any material defect of them, the child must also suffer. This shews the peculiarities of the fœtus, and its connection to the mother;—of whom indeed, in this respect, it may be considered as a part. But it by no means shews, that the human species can exist without the action of the lungs, when turned into the world, and all such assistance is denied it. Some have contrived schemes for the oxygenation of the fœtus; but I shall hereafter endeavour, at least to shew it may subsist by the communication it has with the mother, without any other assistance, in the same manner nearly as any other part of the body. Doing this, I find, will involve me into an investigation of several other important parts of physiology; and therefore I must be obliged to defer it till some other time. There is no necessity for it here; for we have arguments in support of the necessity of the lungs, much stronger than any they can bring against it: As for instance, when their action is denied, the animal will inevitably die; and when their action is only for a short time suspended, circulation receives a sudden check. We have many examples of people dying



dying in coal-mines,—in chambers, from the vapour of charcoal,—in hot countries, from the vapour arising from fermenting wine:—to these I may add, some few of people dying, and very often fainting, upon suddenly opening dead bodies, or any other vehicle containing deleterious vapour:—all these clearly shew the effect of these vapours, upon the nerves, by means of respiration; and whenever the bodies of the persons killed by such things, have been examined, blood has always been found lodged about the heart and lungs, in the manner just now described. Moreover, we have examples of this, the blood being so obstructed in those who have been hanged or drowned; only there is this difference, that those who die from respiring noxious gases, have the heart rendered much less irritable than those who die from hanging or drowning; (see Mr Coleman's experiments concerning these things), a plain proof that these gases diminish the sensibility of the nerves. In all these cases, the blood obstructed on the right side of the heart, is in proportion to that of the left, as 13 to 7.

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#### S E C T. IV.

NOR do we find it is injury of the nerves from defect or suppression of respiration alone, that produces such bad effects; for injury of the nerves of the stomach is quite as dangerous as by a violent blow.

blow, by acrid medicines, or by over-distension, and the like. And when these causes are applied in a less degree, then of course the effects will be less alarming. They are at first a shivering, yawning, and trembling; then great uneasiness, especially in the part where the injury is first applied, both in the thorax when we respire improper air, and in the stomach, from any injury of the nerves distributed upon it: In short, whatever deranges the nervous system in any part is in danger of producing death, when its action is very strong; but when it is very moderate, (as we may all, I believe, many times observe, after overloading the stomach, as well as in ague), it only brings on yawning, stretching, coolness, shivering, and indeed all the symptoms of ague, only in a milder degree. But what keeps up the distinction is, they never return, except when brought on by the same or a similar cause: whereas, although, upon the first derangement of the nerves of the lungs, the symptoms are the same at first; yet, as these nerves are distributed upon arteries, they will produce greater change upon the circulation; and therefore a disorder will be induced more complicated and lasting, which is called the ague, or intermitting fever.

At first it may appear an objection to what we here believe to be the truth, that we are often exposed to those kind of vapours in larger quantities than we meet with them in marshes, and yet not ague is produced. This, as I said before, may be an objection to the matter being of a contagious nature, but will lose its force when we consider the  
disease

disease in conformity to the opinion we have formed of it. For there is this difference in the strength of people's constitutions, that the same air which produces the ague in one person will scarce affect another: and it must be observed, that it is not a temporary exposure to this kind of air, though exceedingly impure, that can bring on with regularity those successive fits of intermitting fevers, but the same kind of air must be respired for a continuance. Yet, when the disease has got fixed in the habit, it will sometimes remain after the original cause has been removed.

We are still farther assured of the effects the air we respire has upon the circulation, from the experiments of Mr Taylor, recorded in the minutes of the transactions of the London Philosophical Society. From these we learn, that the respiration of more than an ordinary quantity of oxigene accelerates the circulation; which also shews, with equal certainty, that the respiration of less than an ordinary quantity, or of other vapours mixed with it, which is the same thing, must retard it, so that the blood will be lodged about the lungs: and this is always the consequence of defect in respiration, as we mentioned before, and as is proved by experience.

But, upon a superficial view, it may not be admitted, that the air in marshes does contain this vapour in sufficient quantity to do so great an injury to the circulation. We formerly observed, it is the continual application of the cause, rather than the

violence of it, that produces ague ; for when it is violent, it no longer produces ague, but some other disease : yet we shall find, that the quantity of these vapours cannot be very small, when we consider the diminution of the oxigene by the turning of it into carbonic acid; the superior weight of that acid, when compared with the atmosphere, which will confine it to the bottom, and the continual formation of the gas hydrogene and azote, the quantity of which, from their rarity, will be exceeding great ; together with the mist in those places, which, if it was nothing more than water, certainly would not be fit for respiration. Dr Fordyce makes mention of a whole army that was upon the point of being routed by this fever, almost entirely in consequence of watery exhalations.

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## S E C T. V.

MUCH has been said about the heart, in explaining the functions of the human body ; and I understand there are many volumes, some wrote, and some now writing, about its structure. All that we can learn at present from Anatomy is, that it is only one part of the vascular system ; only it seems to have more nerves, and being larger, and of a different form, its fibres run in different directions ; and it has greater force. Yet it is by no means wonderful that it is made a subject of so much discourse ; as it is well known,



known, a good genius has it in his power very often to say a great deal about nothing. But what has turned mankind so much to the observation of this organ, is the office allotted to it by some, of carrying on the circulation of the blood, which it was imagined to do by innate power of its own; because, when taken from the body, or even after death in the body, it might be stimulated to motion: In the last case, it may be done by injecting the arteries with air or water, immediately after death.

This shews the nerves of the heart are very delicate and sensible; but if any other part, or only a nerve going into any other part, be irritated in the same manner, immediately after death, the same effects will ensue; and even the electric fluid, running along the nerve, will be sufficient for the purpose. This is a fact first suggested to me by some small though valuable experiments, performed upon frogs; and we may repeat them at any time.

It may not be improper here to observe briefly, before we quit the subject, that there are two opinions about it in physiology, which have generally been opposed to each other; the one is, "the heart" "is stimulated to contract by the blood poured into it, " and thereby enabled to carry on the circulation." Those who profess this opinion (as well as others who think the heart is kept a-going by some peculiar power of its own) think, if they admit the importance of the lungs, their theory must be cast down: but I am not altogether of that opinion; for I must maintain the whole arterial and venous system

has the power of propelling the blood ; yet I allow the heart has that power in a more eminent degree, because it is larger, and the size curvature, and the perpendicular direction of that part of the aorta and vessels immediately proceeding from it, render that the more necessary. But when I have allowed this, I must go no further ; for here, both common experience and experiments shew us, these cannot long continue to act without the assistance of the lungs. There is an experiment made by Dr Langrish, which proves this very decidedly. Dr Whytt has mentioned it, though it certainly tends very much to overthrow his opinion. (See p. 72. of his Work). “ In a  
 “ dog whose thorax Dr Langrish opened, and whose  
 “ lungs he kept playing with a pair of bellows, the  
 “ auricles began the motion, and the systole of the  
 “ ventricles always followed that of the auricles.  
 “ *When he desisted from blowing fresh air into the*  
 “ *lungs, the heart lay still; but recovered its motion*  
 “ *when the lungs were strongly distended anew.*”  
 They who are such advocates for the power of the heart tell us, in such cases death would be owing to the collapse of the lungs ; but they may distend them as much as they please, with any kind of air deficient in oxygene, and they will find the effects just the same.

From these and many other such like experiments, now very common, it is, I think, very plain, not only that respiration supports circulation, (which probably may be a great deal through the medium of the nerves), but that the nerves are possessed of great irritability,



irritability, and in consequence of this they are easily acted upon, both by the mind, and by external things; and when they are acted upon so as to receive a proper stimulus, they give motion to all parts of the body, internal as well as external; so that I should believe, in tracing out the causes of disease, we ought to pay great regard to the power of the nerves.

It is in consequence of this, the tenderness of the nerves, that we presume the effects of those elastic vapours may be accounted for. It would suffice for us barely to mention facts, and so proceed to our deductions, without endeavouring to explain them:—when we can do it briefly, however, we should hope it may not be unacceptable; but when such explanations would lead us from the subject, we shall always beg leave to reserve them for another work.

The present fashionable doctrines of nervous energy, irritability, and sensorial power, &c. may seem to militate against us sometimes; but very often I am inclined to think, our meaning may not be widely different. Our opinion is, that all nerves are capable of being acted upon by other stimuli, besides that coming from the brain; i. e. their nature is the same, and equally irritable with the brain itself. And we are supported in this, by what has already been said, and by what we all may learn from our own observation.

From this, then, it follows, that by the respiration of what has been called phlogificated air, or air in which oxigene is deficient, the usual stimulus is denied

denied to the nerves to carry on the accustomed circulation through the lungs ; for the nerves being rendered less irritable, (as is evident they are from experiments made by Mr Coleman), they do not act so powerfully upon the arteries, and therefore the blood is retarded in its course, and at length accumulated, till it either causes death, or some force is applied to send it off.

Concerning the theory here advanced, too, it may be necessary to speak some other time ; but the facts, I trust, are very plain.

We have all along considered the impurity of the air as the predisposing cause, and very often, too, it may be the exciting cause of agues ; and then they come on very gradually, the fits at first being marked so indistinctly, as to come and go almost unnoticed. But in winter, especially when the weather is often changing, and also at other times, other proper exciting causes are not wanting ; and then it is commonly said these bring on the ague : such, I mean, as improper diet, depressing passions, and obstructed perspirations, from taking cold, &c. ; and these, with propriety enough, may be named the cause of agues : but then they must only be the exciting causes ; for though, probably, the ague might not have occurred had these not happened, yet, too, it must be remembered, that they do occur times innumerable, and produce no ague, except the air at the same times gives and keeps up the proper predisposition.

It is observable, that in hot countries the intermitting fever is always most fatal ; and it is more frequent in moist warm weather. The reason is very evident,

evident, because the vascular system is more irritable in hot climates; and upon any adulteration of the air, as by moisture, &c. to be used in respiration, this is more easily affected. Though Huxham says\*, such an atmosphere is very injurious, (and many others have alledged it is so, by constructing the vessels of the skin); yet he was sensible, no contagion in such cases could be sought for with propriety, and therefore he made the supposition, that it causes these fevers by obstructing perspiration. Yet, when we consider this, we must recollect, in cold weather the skin is far more liable to contract, than it is when agues are most predominant. So that if the perspirable matter is detained, still we are compelled, as it were by necessity, to have recourse to the lungs. And these are so formed by Almighty Wisdom, that in cold weather, when the skin is most liable to be contracted, and so rendered impervious to the matter of perspiration, that then the arteries are more stimulated by the action of the lungs, so that the matter perspired makes its way with greater force. And, on the other hand, when the skin is relaxed by excessive heat, then, too, the vascular system is also relaxed, but may be kept tolerably free from irritation, by acid drink and vegetable diet; because its extremities terminating in the skin, are more dilated to allow a passage for the hot and irritating vapour raised from the blood; and it is not now excited so violently to action by the stimulus of air in respiration. Or, in other words, our bodies are thus so admirably formed, that they are  
not

\* Huxham, p. 18. & 25.

not much deranged by the mere change of temperature, provided we only take care to keep them from injury from other causes.

Sir John Pringle takes another way of explaining the effect warm moist air has in diminishing the discharge of perspirable matter. He says, (p. 184.), in imitation of Sanctorius, " When the air is filled " with these vapours, it admits the perspirable matter " with more difficulty : " as if, being somewhat inclined to saturation, it could not receive any more. But the best and most rational explanation this fact admits of, seems to be, that this air denies the usual stimulus to the vessels in the lungs; in consequence of which, the blood is retarded, and accumulated till it produces those symptoms of cold, shivering, anxiety, &c. by oppressing the nerves distributed on the larger arteries; in the same manner as the same symptoms are produced by oppressing the nerves distributed on the stomach. And in conformity to this, we find that purer air sometimes, by giving the wonted stimulus to the lungs, is relied upon as the safest cure.

Having now gone through the principal circumstances relating to the cold fit, not so accurately, it is true, as we could wish, yet with as much plainness as we could, we shall now consider the second stage, or hot fit of intermitting fevers.



## CHAP. IV.—SECT. I.

THIS is allowed by the most judicious of our medical writers to be only a consequence of the former ; but they will have it, it is a bustle of the *vis medicatrix naturæ*, somehow induced to overcome the disease. We conceive it to be as follows : The blood is accumulated in the lungs, and about the heart, in the manner, and by the means, just now described, till it produces an irritation of the vessels, by the distension it causes of them.

It is proved beyond a doubt, that blood is so lodged about the lungs from impeded respiration ; and it is full as evident, this blood is again carried off into circulation at certain periods, from the increase of action in the vascular system, and from the now great derivation of the blood to the extreme parts of the body. Hippocrates took notice of this, in his book concerning winds ; as indeed what is there in medicine he did not take notice of ? But his theory about it, I confess, appears to me somewhat whimsical. He says, “ Horror, or a kind of distressing sensation, is induced in the whole body, by air absorbed from the intestines, and cooling the fountains, as it were, and origin of the blood. Moreover, the blood, dreading this horror, runs (for protection, I presume), to the internal parts of the body.” This (omitting so whimsical a theory) coincides perfectly well with what we have written, and with what has been found upon dissection. This is the cause

of the cold fit. He then adds, " But much blood being accumulated," (more than usual in the internal parts, where also he supposes it brings on inflammation and heat), " the air itself, which cooled the blood, is now overcome by the heat, and growing hot, and burning as it were like a fire, excites heat in the whole body, and is assisted by the blood." He then says, " As that which is made hot is dissolved, and sends out vapours, so likewise this hot blood sends out a vapour, which breaking out thro' the pores of the skin, sweats arise." Hippocrates all along shews his most accurate observation ; but, as I said before, though his explanations of many phenomena which he had observed appear rather extravagant, yet I cannot but think he may make a conjecture now and then very true, which succeeding writers upon these subjects have not availed themselves of. Such, for instance, as some part of the body being deficient in blood, while the inward parts have too much. This is rather a matter of observation ; only it is the way of accounting for it, and explaining the consequences of it, that here I am going to speak about. What he attributes to the air taken in from the intestines, I would attribute, as I have said before, to the refusal of a proper stimulus from respiration, to carry on the circulation. But when he says, in the same place, some parts will shake from the deficiency of blood, and those where it is too abundant will become inflamed, I believe he is very right. It might be added, they will be affected too, at first, with a sort of dulness and shivering from



from the oppression of the nerves\* ; but too great a stimulus applied to the nerves will certainly cause irritation in the end, and too little debility, which, I have no doubt, by continuing a short time, may bring on a kind of palsy, or shaking of the limbs. This, I suppose, is what gave rise to the sympathy mentioned by Dr Cullen and Dr Darwin, between the internal parts and the skin. I cannot deny, that when one is affected, the other generally is so in consequence of it ; because the exhaling vessels of the skin are no more than continuations of the heart and arteries ; and when the skin is contracted by external applications, it certainly must make greater resistance to the action of the heart and arteries. But here we see the action of the heart and arteries is decreased first ; and, in consequence of that, the discharge must be far less copious than usual by the skin.

As to conglomerate glands, I cannot say much about them. I think some other means might be contrived for letting off the perspirable matter ; for as they never were demonstrated, I rather suspect the excretion of this fluid will, or may at least, be explained without them. Hippocrates proposes a theory on this subject, part of which was mentioned before : But he says, “ This vapour raised from the  
 “ blood is changed into water, (that is, into a watery  
 “ consistence), by the resistance it meets with in the  
 “ skin ; and thus is seen in drops, just as when any  
 “ solid body is exposed to the vapour of hot water,

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“ the

\* The nerves distributed on the blood-vessels are here alluded to ; which are stimulated by the motion of the blood.

“ the vapour is thickened and condensed upon it, and drops are formed, which descend.”

Solar, lunar, and terrestrial gravitations, have been mentioned too, in explaining the phenomena which take place in fever; but they are words in their present sense so vague, and unconnected with the subject, at least according to what we know, so far, that we should be no wiser, even admitting them to be true; except we have recourse to miracles, (which it appears the author denies we ought), and so explain them according to some preternatural laws of action. But if none of these things are to be, I humbly propose we may attempt to explain them after this (or some such) manner, as is followed in this book.

It will not be wonderful, that this irritation of the whole vascular system should be caused by the more than ordinary distension of only one particular part of it; when we consider the many nerves there are terminating in that part, viz. in the superior part of the aorta, and in the heart; and indeed in all parts of the arterial system; it is probable, that there may not be so many in the lungs; yet, notwithstanding that, I judge the vessels they contain are not less capable of irritation, from any unaccustomed distension of them; because, I presume, their want of nerves is counterbalanced, if not more than counterbalanced, by the greater stimulus these nerves receive in respiration. And it is well known, an inflammation in a less important part, will do the same. The stomach (which, from its size and office, and ner-

vous

vous texture, much conduces to the illustration of what we are now speaking off) becomes inflamed, or at least much irritated, from being distended with too much food; the cold and shivering, &c. first occur, and then the irritation, which proceeds from the same cause, only carried to a higher degree; for nerves, when they are only gently compressed, or slightly injured, do produce debility, or numbness, or one or more of the symptoms of the first stage of an intermittent: as is also witnessed by any slight compression of the brain, from fracture of the cranium, or other causes; and by distension of the spermatic chord, from a stone lodging in the ureter, and by various other facts of the like kind. But these symptoms are very quickly over, even when the cause of them is still remaining; and especially, if it is endued with greater force: for fever and delirium, we know too well, too often succeed them in cases of compressed brain; because the part is tender, and of high importance. Inflammation of the superior part of the thigh, and of the testis, succeed that numbness, &c. of the spermatic chord, consequent upon a stone lodging in the ureter; because the nerve affected by it terminates in those parts. And in cases of over-distended stomach, though many nerves are found distributed upon it, and the first symptoms (such very nearly as constitute the cold fit of agues) are very severe; yet they cannot terminate in ague; because for that there is required another cause, viz. one that may not only bring on, but occasion, the return of all those symptoms characterising



characterising ague ; that is, cause the regularity of the changes observed in the disease. Here, when the inflammation has arrived to a certain pitch, upon the stomach, it causes it cast up its contents, and then the inflammation for the most part ceases ;—sometimes, it is true, this inflammation produces fever, when the matter causing it by the distension of the stomach was too acrid, as well as great ; and this fever so induced may be of the most dangerous kind ; yet, as we said before, it must not be a constant irritation of the vascular system, such as here takes place, but only at stated periods, that we know is very necessary for the production of an intermitting fever. Such an irritation, and such alone, as is produced from the respiration of impure air.

That the arteries are very sensible, and contain many nerves,—consequently that they are easily affected by any unusual stimulus, appears from this : If you tie up the artery of a living animal, going to any particular part, an immediate numbness will be perceived in that part, though the other nerves going into it should not be injured : And when a vein is opened in the foot, (probably from wounding some small branch of the anterior crural nerve, as there is one running very near the vena saphena, at the ankle where it is opened), a numbness is very often caused, and even mortification has been the consequence. These things can only be accounted for by admitting the great importance of the nerves, and their distribution upon the arteries ; for although we cannot absolutely trace that nerve, as well as  
many

many more, immediately into the arteries, yet I have no doubt they do ultimately spread themselves upon them. The muscles can be nothing more than a certain intexture and disposition of the arteries; for we find, that not only the blood flows from them when they are punctured, shewing that there is a direct communication between them and the arteries, but also that blood is no where found without making such a puncture; which, as I conceive, confirms that the blood circulates through the muscles, and must of necessity take such a course.

I do not now believe I have discussed this subject so much as I really might, but I believe enough has been said to shew, that the nerves must terminate in arteries at last; which in fact is no new opinion, for it was mentioned by Cowper, as well as other authors: "To the nerves *the muscles, as well as the* " *other parts of the body*, owe their sense, and likewise their motion." (P. 4. of his Introduction to his Anat. Treatise). And as we know no other media of sensation or perception in the human body, we must admit, when their communication with the muscles is destroyed, the muscles must become incapable of stimulus from circulation, as well as from any other cause; and, in consequence of that, the blood will stagnate, receiving no assistance from the arteries, any more than it would from passing through mere lifeless tubes; which in reality they now are, and mortify, and return to their original dust. This, moreover, shews the heart of itself would be incapable



pable of carrying on the circulation; and even when assisted by the lungs; the arteries themselves must be endued with the same contracting force.

To make what has been said the more plain and intelligible, it is necessary we should declare what we mean by nerves receiving their proper degree of stimulus or irritation: We mean, that the nerves are then rendered capable of acting upon the muscles, or the arteries, so as to produce such motions as we desire, or to carry on the vital functions in a manner consistent with good health. And when they receive more than this degree of irritation, either from the brain or from any other source, then inflammation will succeed, and probably delirium, from their preternatural, and somewhat convulsive kind of action. But if they receive less than their proper stimulus, (for so long as life remaineth they must have some), then debility, or syncope, or palsy, &c. are the consequences.

To sum up what has been now advanced concerning intermittents, it is this: *Blood is accumulated about the heart and lungs, from the impurity of the air; this accumulation is the exciting or more immediate cause of all the symptoms of the cold fit. Then, from the irritation of that very important part of the vascular system, the blood is violently driven off again into circulation; and this we call the more immediate cause of all the symptoms of the hot fit.*

The cause of the first stage of this fever passing into the second, we have said, is owing to the peculiar nature of the nerves; and in explaining the movements

ments in the animal body, we judge they ought in the first place to be considered ; yet we must recollect also, that there are other sources of motion in animal as well as in other bodies ; though their *modus operandi*, I believe to be more plain, so that we may more readily comprehend it. If we take any elastic tube, and stretch it beyond its usual pitch, it will recoil ; just so do the arteries about the lungs, after being more than usually distended by the blood ; only there is this difference, arteries have the advantage of being surrounded by the nerves ; which are enabled to give a check to the distending force.

I must here again beg leave to refer to a passage in the book just now mentioned, of Dr Whytt's, p. 19. He says, " Whatever stretches the fibres of any muscle, so as to extend them beyond their usual length, excites them into contraction ; almost in the same manner as if they had been wounded by any sharp instrument, or acrid liquor." And he gives an example of this : " The heart of a pigeon newly dead may have its motions renewed by stretching the great vessels to which it is attached." But at p. 53. after mentioning several circumstances, which make what he then says extremely probable, if not quite certain, he concludes, (which is more to our purpose), that " even the distension of hollow muscles has a remarkable influence towards exciting them into action." Another author of our own times, expresses the same opinion, in the following manner : " Muscles mani-

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" festly

“ festly possess, like other solid parts of the human  
 “ machine, a great degree of *elasticity*, that is, the  
 “ power of *re-assuming their own form* when stretched  
 “ beyond their natural limits by any distending force ;  
 “ which power continues even after death. What-  
 “ ever other motion they have more, that de-  
 “ pends on life, and the connection that is formed be-  
 “ tween them and the brain and nerves.” (See Pugh  
 on Muscular Action, chap. 4. p. 34.).

From the blood being so repeatedly detained about the lungs, and perspiration stopt in consequence of it, it must be loaded with a more than ordinary quantity of impure matter, and become more black, which it generally does, as is found upon dissection ; and Cleghorn mentions, in his Diseases of Minorca, these fevers often run on into a putrid kind ; in consequence, I suppose, of this alteration of the blood, and the warmth of the air.

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## S E C T II.

It is to be here observed, that this disease sometimes proves fatal by other means than by causing a putrescency of the blood ; for people’s constitutions being very different, that is, the strength and firmness of their arterial system, the same state of the air which brings on only a mild intermittent in one, may cause it to be very severe in another : and the blood which has been so accumulated about the vessels of the lungs, may be impacted there, the vessels being



being incapable of sending it off again into circulation;—or they may do it with so much force, from their now great degree of irritation, that a vessel may be ruptured in the lungs; or lastly, by their endeavours to send it off, convulsions may be excited; so that it seems very probable, death may be caused by this disease, in four different ways.

But when these things do not happen, the hot fit comes on, characterised by “a strong, large, and  
“more free respiration; less anxiety; a larger and  
“stronger pulse; great heat, thirst, and redness, and  
“great pain in the head, and limbs, the urine being  
“generally red;” and this stage (Boerhaave observes, see § 750.) answers to the achme or height of continual fevers.

Van Swieten, in his Commentaries upon this aphorism, very rationally attributes this greater freedom of respiration “to the blood, which before was almost stagnated about the heart and lungs, having  
“now a freer passage, and rapid motion.” But afterwards says, that “this stronger respiration is necessary, in order to transmit a larger quantity of blood  
“through the lungs, in the same space of time;” not considering that when this “almost stagnated  
“blood” was removed from the vessels about the heart and lungs, the respiration would be easier of course. Again, instead of following the removal of this almost stagnated blood, through all its consequences, he loses sight of the subject entirely, and accounts for the other effects of it; such as less anxiety, a larger and stronger pulse, &c. by saying,

“ the heart now propels its contained blood into the arteries, which are now pervious in their extremities ;” intimating that they were not pervious before, or that there was (what Dr Cullen calls) a spasm of the extreme vessels. But as all these things may proceed from the afore-mentioned circumstance, viz. from the blood retarded about the lungs, being again driven off into circulation, from an irritation it causes of its containing vessels, I see no occasion for any other supposition. This irritation has (and not improperly) been called the re-action of the system.

It is mentioned also in the same Commentary, (which I repeat only, because it is a good example of the common opinion about this matter, conveying no distinct idea), that “ in acute fevers, nature sometimes overcomes the disease,” (by expelling the morbid matter), “ and sometimes is overcome by it.”—But “ in intermitting fevers we are certain, that in this fit nature will prevail ; since those who die, perish in the cold fit.”

It has been, and now is, a matter of surprise, why nature should be so much spoken of by physicians. From its proteus-like qualities, one should be apt to suppose it a being of considerable activity. And physicians in general, I believe, agree in ascribing to it very great power, which it may have for any thing I know ; but, I am persuaded, nothing more ought to be understood by it here, than that in some acute disorders the irritation of the vascular system becomes greater than our frame can bear ; on which account,



account, death is the consequence. And concerning intermittents, nothing can be feared of this kind in the hot fit; for what would bring on such a violent irritation, so as to cause death, if it fully took its course, would also so derange the nervous system, that death would be the consequence before such an irritation could take place. Life in such a case, I should expect, would depart by apoplexy, or hemoptysis, or suffocation, which has now and then been observed to be the case. It may happen also, especially to people of a lax habit, and to children, that their arterial system may be so weak and irritable, as not to be able to propel the blood, and then they may die in convulsions.

The other symptoms of this second fit, or hot stage of the fever, such as pain in the head, sometimes amounting to delirium, and redness of the urine, must always follow a too rapid circulation; because the impetus of the blood will be too great for the delicate structure of the head, to bear without injury; and the secretions, as well as excretions, must invariably change, with every change in the circulation:—So that I need not say any more concerning this, but proceed to the next, which is the third stage of an intermitting fever.

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### S E C T. III.

THIS stage is ushered in by “ profuse sweating; “ then a remission of all the symptoms; the patient  
“ falls

“ falls into a sleep ; the fever goes off, but leaves a  
 “ weariness and weakness of body.”

The increased action of the arteries (as might be expected) forces the blood towards the surface of the body, and its finer parts through the exhaling vessels of the skin.

When this preternatural exertion of the vascular system has subsided, from the exciting cause being for a time removed, if it could not be removed, this exertion could not last long, only the termination would be different, it would end in death ;—but, as was said before, having for a time subsided, greatly assisted by a discharge of the very acrid stimulating matter of sweat, the body must return somewhat nearer to its state of health ; only a languor and weariness remain, merely from the violence of the preceding fits ; or if we have a mind to express it so, from the energy of the nerves, being too much weakened by their late, strong, and preternaturally excited exertion.

This weakness and weariness, together with a continuance of the first and greatest cause, viz. respiration of impure air, dispose the body for a fresh attack ; and it may not perhaps be improper to remark, that the frequency of these attacks, as well as their violence, (independent of accidental circumstances), will always be governed by the greatness of the predisposing cause ; and the strength and firmness of the vascular system, upon which it has to act : Thus, we see, some are subject to tertians, and some to quartans, &c. and even at different periods,

periods, the same person may meet with both. In the above-mentioned manner, the disease goes on, either till the cause is removed, or till the vascular system is strengthened, to resist its attacks; or (which is very often the case) till it is so irritated by the frequency of the fits, that the disease terminates in continued fever.

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## CHAP. V.—SECT. I.

### *The CURE.*

**S**INCE what precedes is for the most part new, and probably may meet with some objections, it would not, I think, be right to speak with confidence, concerning any alteration in the method of cure. We are all liable to mistake, and if the theory here laid down should be wrong, (which possibly it may be, though I am inclined to hope it is not), all practice built upon it must of consequence tumble to the ground. I shall only therefore, at present, discourse upon the cure, in a very general manner.

Hippocrates mentions, it was a great difficulty in his time, to determine in acute diseases, what sort of medicines would be most proper, those called cordials, or the contrary\*: and the same difficulty still remains; for some even now dispute for purgatives,

\* Concerning diet in acute diseases.



gatives, while others cry out as violently for cordials. But in this fever, I apprehend, we must not too much adhere to either practice; for being divided into three distinct stages, all materially different from each other, it will not be difficult to conceive, they must require very different treatment: Yet at the same time, as they are all dependent upon the cold fit, our endeavours ought chiefly to be bent towards the removal of it; and if we succeed, the others will disappear of course.

The medicines best calculated for this purpose seem to be, all those which give strength and firmness to the vascular system, that it may resist that tendency to obstruction; such as, bark, or stimulating medicines, (before the fit), or iron, or opium, and red wine. Opium is found, from some experiments made by a respected gentleman of this place, to quicken the pulse, by bracing the muscular fibres of the heart and arteries, and so causing their contraction at shorter periods. Bark, I suspect, acts pretty much in the same manner; only its effects appear to be more gentle, and to continue for a longer time; for this reason, it certainly ought, in this disease at least, to have the preference. Wine makes the pulse also more full; and therefore, for some constitutions, (such as are much exhausted, but not much inflamed), it appears better calculated than the other two. But it is quite evident, different constitutions require great difference in the manner of treatment; and to enter here more minutely into the subject, would swell the work much larger than



I intend. However, I think we may safely say, there are no constitutions but may be freed from this disease, (except they are quite worn out), by some medicine of the tonic class, which may strengthen the arterial system, and so prevent any accumulation of the blood about the heart and lungs; provided they use exercise at the same time;—which undoubtedly must be considered almost absolutely necessary: for we all know, by exercise the arteries receive greater energy, and are excited to greater action; and without it, the firmest body becomes weak; so that it is reasonable, we may expect it will of itself go a great way to effect a cure.—Celsus advises it should be used before, and at the time when we expect the fit, (Book iii. chap. 15.).

The respiration of purer air also, it is observed, will very often effect a cure; for then the cause of the complaint is actually removed: Yet it often happens, when the system is weakened, and the body habituated to those changes, that it is a long time, even then, before it can acquire its usual vigour. Exercise and tonic medicines may still be required to give their assistance.

But it must here be remarked, that though these medicines have the power of giving strength to the circulating system, and of consequence to the body for a certain time; yet the system becoming used to such stimuli, they at last produce no effect:—what I would here suggest is, that they ought never to be used, but in cases of emergency; and some practi-

tioners are so cautious in this respect, that they will scarce admit them to be used at all.

For shortening the duration of the cold fit, when it actually is come on, some authors advise emetics; and, for ought I know, they may answer the purpose very well: but, at the same time, they distress the patient very much; they weaken the body, and (if before the patient was subject to hemoptysis, or particular weakness in any important part) their operation is attended with no inconsiderable degree of danger. Besides, it is presumed, their efficacy here consists chiefly in the shake (as it is termed) they give the constitution; whereby the blood, which was just now beginning to stagnate, is again pushed forward in its course. When they have gained this part of the victory, they follow up their advantage with the bark. But this practice, however successful it may be, when faithfully adhered to, is not much calculated for general use: for, independent of those who would neither profit by bark or emetics, there is, I believe, a still greater number who would very strongly remonstrate against the disagreeableness, and sometimes even danger, of the remedies. If we could find others more safe, pleasant, and effectual, we should, I am persuaded, do them, and indeed the world at large, a very acceptable piece of service. I would propose electricity, and the respiration of air, containing a large quantity of oxygene. But it is necessary to add, that it is only probable, that these may prove so valuable, for they have not at present,

present, as far as I know, been sanctioned by much experience.

The cold fit having terminated, our attention must now be directed to the hot one ; which will be, with respect to violence, always proportionate to the first : Yet, notwithstanding this, it must be noted, that any very acrid medicine being given in the first fit, to stimulate the system, and remove the obstruction, though it may succeed in this, yet it will cause the following fit to be more severe. This again points out the danger of any powerful stimuli ; moderate doses only should be used, and those of medicines whose effects will soon be over. Opium and wine seem very proper for this purpose, especially a well-proportioned mixture of them both.

The management of the hot fit is now so well known, that it is almost unnecessary for me to say any thing about it. The body must be kept cool, plenty of diluting acid drink should be given, but not in very large quantities at a time ; and (what is of importance) particular care should be taken that the secretions and excretions should not be retarded by obstruction. When this is not attended to, the general inflammation will be augmented, and a new one will very often arise in some particular part. Diluents of an acid nature are more proper, because these not only increase the quantity of the fluids, and in that respect tend to bring on perspiration, without much debility, but they also tend to strengthen the tone of the arterial system, to cool

the fluids contained in it, and to resist their now newly-acquired tendency to putrefaction.

This may perhaps be questioned by some; but my opinion is, Sir John Pringle's experiments are by no means such as to enable us to say with any certainty, which are putrescent medicines, and which are not; there being great difference between the effects any body may have, when turned and acted upon by the animal machine, and those effects it produces upon a more inanimate piece of flesh. Alkalis undoubtedly do cause putrefaction:—to be convinced of this, it is only necessary to refer to the book Huxham wrote on Fevers, (page the last), where he relates the case of a man, who brought on that putrid state of the humours, characterising scurvy, merely by eating alkaline salts; and therefore I suspect it may be the retention of the perspirable matter, in some degree, causing the quicker change of the blood, stagnating about the heart and lungs, together with the great heat and action of the vascular system, which cause these fevers sometimes to change into a putrid kind, as mentioned by Cleghorn, in his Diseases of Minorca, and Sir John Pringle. However, this is certain, that all these circumstances very much aggravate that putrescency, when it has begun, and also are injurious in this disease, and the acid, and astringing liquors appear best calculated to prevent and oppose them.

There are many irritating medicines used to bring on perspiration, such as antimonials and the like: Purging also, and bleeding, have been in great repute;



pute ; but concerning these things we will beg leave to speak hereafter. After diluting and acid drinks, it is of importance to keep the body tolerably cool : But I think moderation should be used in this matter ; for if they are suddenly exposed to great cold, or kept in it too long, a constriction may be brought upon the vessels of the skin, which may be followed with no small degree of danger ; but it will hardly be possible for a prudent practitioner to err in this respect, because the feelings of the patient, I believe, will be his safest guide.

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## S E C T. II.

WHEN this preternaturally excited impulse has spent its rage, by a discharge of the more hot and irritating parts of the blood, a weakness must necessarily be left behind. And this (as is well known) is best removed by the cardiac division of the materia medica, and by wine.

I think, in the administration of these medicines also, the feelings of the patient are to be peculiarly consulted ; for though there are many medicines, which in general practice answer very well, yet to many particular patients they will not answer. Wine is the most universally liked, and on that account is most valuable, yet even this as a cordial will sometimes fail, but more seldom if it is mixed with some grateful vegetable acid, as lemon-juice : this I esteem the best of cordials when properly employed ;

ployed ; and the quantity should not be large which is given at a time, the doses ought rather to be often repeated ; care should be taken that no more may be given than brings the patient to a composed and grateful state of mind. When the quantity of acid is pretty large, and the mixture much diluted, any unwished-for effects from too much wine will seldom happen. The tincture of bark given with it, I can say from experience, has appeared to me a very valuable addition.

Having now gone through the three stages of the disorder, as concisely as I could, and pointed out in general terms only the treatment I judge most proper for each, I shall just take a short view of medicines, and the method of cure frequently had recourse to, in inflammatory disorders.

### *Vomiting and Purging.*

By evacuating the stomach or bowels, a violent commotion is raised in the constitution, the stomach or the intestines are relieved from the pressure of the food or fœces, and, I believe, to speak in general terms, these are the only effects produced by these medicines. Now, as was said before, the violent operation of any medicine, may, by the pain it gives, excite the nerves to action ; for, according to Dr Darwin and a few others, (Dr Whytt, I believe, expresses the same opinion)\*, convulsions are brought on by the action of the nerves to relieve some pain, or are “ efforts to relieve pain,” (*Zoonomia*, Diseases of Volition, class 3.) ; therefore, when

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\* P. 18. and the following.

any pain, flighter than that required to induce convulsions, is applied, the nervous power may in like manner be called into action, but not in so violent a degree ; this action may be barely sufficient to remove obstruction, and, in this disease, to terminate the cold fit, by propelling the blood now beginning to accumulate in the larger vessels about the heart ; so far they are of service ; and vomiting will have greater effect than purging, because the place where it more particularly excites this action is nearer to the seat of the disease. But our view must be directed to something beyond this ; they, for the most part, are succeeded by debility, which, in this disorder, is always unfavourable, (which will very often be the case when the causes of the disease still remain) ; and if they do not happen to succeed, they certainly do much harm, by inducing this debility, inasmuch as it will give the disease greater hold ; but besides this, there is another consideration, if they are to be often repeated, they are almost as bad as the disease itself.

Yet, notwithstanding these objections, when the stomach appears disordered, a gentle emetic will certainly be found useful ; and when there is any irregularity in the intestines, (which may proceed from debility, or from irritation, or from the nature of their contents), the nature of it should be well understood, and then the means of obviating it will be suited accordingly ; diarrhæa and costiveness are, I judge, about equally unfavourable.

*Venesectio,*

*Venefectio,*

Is the refuge (tegmen ignorantix) of every quack; and yet physicians of the greatest eminence are hardly able to determine when it might be used with advantage. When there is an unusual dullness, almost stupor, observed in a person, who otherwise appears in tolerable good health, and full of blood, I should be apt to think bleeding might give relief, and prevent the evil consequences of an approaching illness; for very often, from full diet and an inactive life, the blood becomes so superabundant, (the evacuations being small), and the vessels containing it at the same time relaxed, that the same symptoms of dullness, and the like, are produced from distension of the blood-vessels, as are produced from a like distension of the intestines, from too long retained fœces, or from too full a stomach. Also, when any increased violence of action (termed inflammation) has taken place in any particular part, or in every part, (then termed fever), phlebotomy may be used, except this inflammation is brought on by some cause which still remains, and which phlebotomy cannot alleviate or remove. Respiration of impure air, great pain, which is likely to be of long continuance, and the like, are such causes as are here alluded to; and it will appear quite evident, that by bleeding, in order to remove inflammation when brought on by these causes, we may draw off all the blood from a patient's body, and be no nearer accomplishing our purpose at last, except it be to kill; and such treatment, if it did not give the disease a superiority, by diminishing the vigour of the body that opposes



opposes it, (which I apprehend might very easily be the case), yet it would lay the body open to many other diseases which proceed from debility, a very formidable train ! and which in the end would terminate in death, even admitting the patient should overcome the original disease. I say not this to depreciate the importance of blood-letting, only I would have it performed after mature deliberation.

### *Sudorifics,*

ARE, I will allow, medicines of very great importance ; yet ought by no means, in my opinion, to be exhibited so freely, or indeed so incautiously, as they hitherto have been ; at present they seem to be employed with more discretion : it is only a circumstance of little moment to hear your patients tell you, if they can get a sweat, they shall soon be well ; they very often may be right ; but to see them weltering in their sweat, (as was formerly the case), in order to cleanse them from “ morbid matter,” as it was termed, was in truth both serious and deplorable. There is, I have no doubt, morbid matter generated by disease, and especially by fever ; but it is merely the product of the disease, and not at all existing in the body before it, (I except the small-pox, the chicken-pox, and the measles, because their production is very different, and I am much in doubt whether I ought to except any more). All common inflammatory disorders may, I believe, be manufactured into malignant ones ; and also, on the other hand, I believe, malignant fevers, by timely

attention, succeeded by judicious management, may be prevented from running any further than common nervous or inflammatory fevers.

But extreme debility is what is induced by excessive sweating; whereas, if care was only taken to keep the patient cool, and to moderate the action of the heart and arteries, which in fever are always too violent, the patient would be prevented from losing strength, and the disease in time would gradually vanish, either from the cause being removed, (which is any thing that may produce inflammation), or, as was said before, from the constitution being made too firm to be inflamed by such a cause.

An easy flow of perspirable matter gives relief, because by that the action of the system is abated, if it comes on after the hot fit; the fluids circulating in it being allowed a passage, so that they may not keep up the irritation by their obstruction. So likewise is it useful to excite it, or to endeavour so to do, to carry off the cold fit; for if we can only derive the blood from the internal to the external parts, we remove the fit: a moderate degree of swiftness in the motion of the blood, gives a gentle stimulus to the vessels containing it, which is of course communicated to the whole body; and thus the shaking, &c. are removed.

The principles here expressed are by no means new, for Dr Whytt expresses himself thus: "The nervous power is *immediately necessary to muscular motion*, but the arterial blood seems to act only in a secondary or more remote manner." And this also  
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is an assertion which he appears tolerably successful in proving, (see his Essay on the Vital and Involuntary Motions, p. 12.).

A gentle perspiration also carries off a cold, and so prevents fever, because it does the same thing which would be accomplished by fever, only in a gentler manner. In fact, all sudorific medicines may produce fever, when given to excess; and the action they bring on in the circulating system is the same as that constituting fever, only weaker in degree. And to give any strong sudorific medicine, so that it may operate at the same time a fever does, is, in my opinion, to increase that fever, and to cause the body, after the fit is over, to labour under much more debility. But that is not all; for if the inflammation before was violent, they will beyond a doubt increase it; so that delirium, and even death, may succeed, before a sufficient degree of perspiration can bring relief.

In short, it may be seen it is a very easy matter to give medicine, and sometimes too with success; but to give it with judgement, is the greatest difficulty; and to be well qualified for that, is no easy task: for concerning medicines, it is not enough that we know their general effects, but we should know something also concerning their manner of producing them. By repeated trial, we may know a great deal: and to conclude, I think I do not know a person who made better use of this opportunity, for trial, than Dr Cullen, to whose work, for further information, the reader must refer; it is truly valuable, and on that account, it is confessed,

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he does receive with justice, the grateful acknowledgement of his country.

I have now, I believe, said all upon this subject I originally intended, and something more; but I have endeavoured to be as plain as I possibly could; and if that has led me at any time to be tedious, I have reliance enough on my readers generosity, to be persuaded they will grant me pardon; not for that only, but for all other defects and errors which they may chance to meet with. I do not think there are errors of any great importance; such, I mean, as may lead us wrong with respect to practice: those I have been particularly anxious to avoid; but that there are many deficiencies, is a circumstance too probable for me to contradict:—Some other time, however, I hope to be enabled to supply them: my present opportunity, together with their novelty and peculiar difficulty, not allowing me at present to proceed any farther. Therefore, to conclude, I may just mention, (which I believe to be the duty of every Author), that whatever is found wrong, I shall recall, with no less willingness than I now have to publish it: and I am convinced, on that account, no man of candour will roughly censure, because that can do no good; and, it is much to be feared, it may do harm, as it probably may discourage all future attempts.

F I N I S.

